This verse is not free from difficulties, and in some parts my translation may be questioned. But it is clear in the main that the poet in praising the Sindhu (the river Indus), mentions its tributaries. The first tributaries which join the Indus before its meeting with the *Kubhâ* or the Kabul river cannot be determined. All travellers in these northern countries complain of the continual changes in the names of the rivers, and we can hardly hope to find traces of the Vedic names in existence there after the lapse of three or four thousand years. The rivers intended may be the Shauyook, Ladak, Abba Seen, and Burrindoo, but one of the four rivers, the Rasâ, has assumed an almost fabulous character in the Veda. After the Indus has joined the Kubhâ or the Kabul river, two names occur, the *Gomatî* and *Krumu*, which I believe I was the first to identify with the modern rivers the *Gomal* and *Kurrum*. (Roth, Nirukta, Erläuterungen, p. 43, Anm.) The Gomal falls into the Indus, between Dera Ismael Khan and Paharpore, and although Elphinstone calls it a river only during the rainy season, Klaproth (Foe. koue ki, p. 23) describes its upper course as far more considerable, and adds: Un peu à l'est de Sirmágha, le Gomal traverse la chaîne de montagnes de Solimán, passe devant Raghzi, et fertilise le pays habité par les tribus de Dauletkhail et de Gandehpour. Il se dessèche au défilé de Pezou, et son lit ne se remplit plus d'eau que dans la saison des pluies; alors seulement il rejoint la droite de l'Indus, au sud-est du bourg de Paharpour." The *Kurrum* falls into the Indus North of the *Gomal*, while, according to the poet, we should expect it South. It might be urged that poets are not bound by the same rules as geographers, as we see, for instance, in the verse immediately preceding. But if it should be taken as a serious objection, it will be better to give up the *Gomatî* than the *Krumu*, the latter being the larger of the two, and we might then take *Gomatî*, "rich in cattle," as an adjective belonging to *Krumu*.

I have dwelt longer on this point in order to show how much has to be considered before we decide on the Aryan or non-Aryan character of local names in India. General Cunningham writes:—

> "The name of Kophes is as old as the time of the Vedas, in which the Kubhâ river is mentioned as an affluent of the Indus; and as it is not an Arian word, I infer that the name must have been applied to the Kabul river before the Arian occupation, or, at least, as early as B.C. 2500. In the classical writers we find the Khoes, Kophes, and Khoaspes rivers, to the west of the Indus, and at the present day we have the Kunar, the Kuram, and the Gomal rivers to the west, and the Kunhar river to the east of the Indus, all of which are derived from the Scythian *Ku* 'water.' It is the guttural form of the Assyrian *hu* in Euphrates and Eulæus, and of the Turki *su* and the Tibetan *chu*, all of which mean water or river."

The Ku in Kubhâ admits, as we saw, of a far easier interpretation. The *Go* of Gomal is the Sanskrit *go*, "cow," and the *Ku* of Kuram or Kurrum is the first syllable of *Krumu*, which is derived from "*kram*," to stride.

Although on minor points like these, and particularly on linguistic questions, some of General Cunningham's statements are open to criticism, the book as a whole is a valuable contribution to our knowledge of the ancient geography of India, and we hope that this first volume will soon be followed by others.

MAX MÜLLER

OUR BOOK SHELF

Epilogo della Briologia Italiana. Del Dottore G. de Notaris, Professore di Botanica e Direttore dell' Orto Botanico della R. Universita' di Geneva. (Geneva, 1869; London: Williams and Norgate.)

DR. DE NOTARIS is so well known in this country by his numerous works on mosses and microscopic fungi, as well as by his liberality to correspondents, that it was with great pleasure that we received the noble volume before us, published at the request and expense of the Commonalty of Geneva. It was not to be expected that a country like Italy, where the borders of the Mediterranean are not rich in mosses, should present much novelty, the more Alpine parts yielding very much the same species as the Alpine or more temperate parts of the European districts. It is, however, always interesting to compare the floras of different countries, even where species are so widely spread as the lower Cryptogams, and it is no matter of surprise to find that there are here very few genera which are not amply represented in our own flora. The only genera which have not at present occurred in this country are Lescuræa, Habrodon, Anacamptodon, Fabronia, which is essentially a genus of warmer climates, Dubyella, Oreas, Pyramidium, Conomitrium, Oreoweisia, Septodontium, Angstrœmia, Trematodon, Braunia, Coscinodon, Bruchia. Most of these are genera either containing one or very few species. The following European genera, excluding those found in the British Isles, seem not to occur in Italy: Voitia, Sporledera, Pharomitrium, Eustichium, Pyramidula, Psilopilum, Anisodon, Platygyrium, Thedenia, most of which contain only a single species. The only genera of the British Isles which do not occur in Italy, are Daltonia, the single species, *D. splachnoides*, being confined to one or two localities in Ireland, Œdipodium, Discelium, Bartramidula, Anomobryum, Tetrodontium, Glyphomitrium, Hedwigidium, Anodus, which again are genera for the most part of one species only, so that Italian muscology cannot be considered as essentially different from that of other European districts. There are undoubtedly many good species which do not occur in this country, but it is probable that the number of these will be much reduced, one of the most curious, *Buxbaumia indusiata*, having been found by Dr Dickie at Aboyne in Aberdeenshire. It is much to be wished that some Italian botanist would give a similar work on Italian fungi. The truffles and puffballs of Italy have been admirably worked out by Vittadini, and something has been done for the more noble fungi by Viviani and others, but we ought to look to Italian mycologists for the identification of the fungi of Micheli. There is no doubt that any skilled mycologist would be well rewarded by the investigation of the Italian woods, which doubtless contain numerous interesting species. We must, however, look to the Italians themselves for information, as many difficulties would stand in the way of a person not intimately acquainted with the language of the peasantry. We see no reason why as perfect an enumeration of the fungi should not be given, as that of the Italian mosses now before us.

M. J. BERKELEY

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Thickness of the Crust of the Earth

ARCHDEACON PRATT has given just the answer I expected to my remarks on his defence of Mr. Hopkins. As I said at the time, I scarcely thought it possible that he could have fallen into the mistake of supposing that the disturbing forces to which precision and nutation are due act by fits and starts. But note what follows from this. His whole defence of Mr. Hopkins's method

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falls to the ground. The very life and soul of that defence was, in almost his own words, that the disturbing forces produced the motion due to their action before friction *had time* to act; or, in other words, that the disturbing forces gave a pull so sharply and quickly that they did their work before friction, which the Archdeacon looks on as rather a sluggard, could rouse itself and counteract them, that they were, in short, able to steal a march on friction each time they gave a pull or a push.

Now it is clear that this explanation has no meaning in it, unless the action of the disturbances is intermittent. Archdeacon Pratt admits that he never supposed that this is the case, therefore he must find some new line of argument, if he wishes to continue his chivalrous defence of his old friend and, I believe, tutor, who can no longer speak for himself. I may add that all who knew Mr. Hopkins personally, and even those who, like myself, only knew of him through common friends, will appreciate and admire Archdeacon Pratt's championship, even if they are unable to agree with him.

In his last letter (NATURE, August 31) Archdeacon Pratt has given a new and independent method, which leads him to the conclusion that the earth is solid from surface to centre, or nearly so; so that if we accept his reasoning we must admit that, whatever may be said of Mr. Hopkins's method, his results at last are right. He mainly rests his argument on the consideration that such a limp thing as an earth with a crust not more than 100 miles thick could not stand the strains with which the disturbing actions of the sun and moon are for ever trying it.

How will it be, though, if we can show cause for believing that the crust of the earth is after all somewhat of a limp thing, and yet does stand these strains? All *a priori* reasoning must give way to fact, if that fact can be established; and though we may be surprised that so thin a crust is able to hold out against the violent treatment it has to undergo, yet, if we can show good reason for believing that the crust is, after all, thin, we must cease to wonder, and try to explain the seeming anomaly.

I shall content myself now with putting forward one of the several grounds on which the thinness of the earth's crust can be, I will not say established, but rendered highly probable; and if my arguments shall prove in the end to have any weight, I have no fear that the seeming contradiction between them and the reasoning of Archdeacon Pratt and Prof. Thomson in the other direction will sooner or later be explained away.

Everyone is familiar with what is known in Geology as Upheaval and Depression, that over and over again during the earth's lifetime portions of the solid crust have been raised, and others lowered relatively to a fixed datum, such as the sea level. Very naturally the idea springs up that the displacement is produced by a thrust acting vertically upwards, or by a removal of some vertical support below. Some cases of small local upheaval may have been brought about in this way, but this is not the machinery by which nature has acted on the large scale.

The fact of upheaval is brought home to us when we find strata originally formed beneath the sea now high and dry far above its level; how the upheaval was brought about we learn by recollecting that these strata were originally horizontal, noting whether they are displaced from that position, and, if so, after what fashion the displacement has taken place. Isolated observations show us strata in some places horizontal, in others inclined at different angles to the horizon; but when we combine into one view a large number of such observations, the result is that we see that the strata have been folded into troughs and arches, that when we find horizontal beds we are on the summit of an arch or the bottom of a trough, where inclined beds appear we are on the slopes. Further, we invariably find that the crumpling up of the strata has been most violent in those parts of the earth's surface which have been raised highest above their original position, that is, on mountain chains. We come, then, to the conclusion that the way in which upheaval has been produced has been by a folding of the strata into troughs and arches. That the crust of the earth, instead of being a rigid unyielding mass, has been from time to time bent into folds, and, so to speak, crumpled up and wrinkled; that it is not unlike, what it has often been compared to, the shrivelled skin of an old dried apple.

Again, the supposition of a thin crust and an internal molten nucleus, gives a very satisfactory explanation of the way in which the crumpling is produced. As the nucleus cools it contracts, and the crust has to accommodate itself to the diminished support within; it cannot shrink, and therefore it gets crumpled, just as in the case of the apple, the inside shrinks more than the skin, and the latter in consequence wrinkles up.

Here, then, is an argument in favour of no very great thickness and a certain amount of limpness in the earth's crust, and it is not the only one of its kind; on the other side, are Archdeacon Pratt's and Sir W. Thomson's weighty reasons in favour of its rigidity. Far be it from me to attempt "tantas componere lites." I only wish to show that there are two sides, and two very good sides, to the question.

What Mr. Hopkins has done seems to me to amount to this: he has shown that with a solid earth the amount of precession would be almost exactly what it is; but he has not shown that this would not equally be the case with an earth having a thin and a *viscous* melted interior; that case he has not attempted to handle, the case he did examine being that of a thin crust and a perfectly fluid interior. If it can be conclusively proved that the thin crust and viscous melted interior are incompatible with known astronomical and mechanical phenomena, we must give them up, but till that has been done we are bound to remember that, whatever has been said against them, there is something in their favour also.

A. H. GREEN

Barnsley, Sept. 8

Temperature of the Sun

I HAVE just seen the interesting note of Mr. Ericsson in the number of NATURE for July 13 (p. 204), and I am very glad that, this question should be thoroughly ventilated.

Mr. Ericsson and others have been startled at the high degree of temperature at which I have arrived, and the appellation of *extravagant* is not spared. I beg leave, however, to observe that this conclusion does not materially differ from that obtained by Mr. Waterston. I am surprised that my opponents, satisfied with rejecting the result as extravagant, do not examine if the method is correct or not. The only objection that could be made is, that, while from the experiments of Soret, the resultant figure is 5,334,840° C., I doubled it, on account of the absorption which the radiation suffers in the solar atmosphere, whose integral effect is a great deal larger than Mr. Ericsson supposes; and it would not be waste of time to discuss the experiments which prove that the absorptive power of the solar atmosphere is very considerable. Mr. Ericsson passes over this too slightly, saying that this absorption would be only 0·01 of the whole, while I have found it considerably greater.

Mr. Ericsson refers to the explanation which I subjoined about this high temperature, that it is to be regarded as a *virtual temperature*, as if these were words *which I would not attempt to explain*. The explanation was, however, given very clearly in my own book, perhaps too shortly, since it seems not to have been understood. The word *virtual* was also employed by Mr. Waterston to indicate the degree of temperature which would be produced in a thermometer by the accumulated radiation of different transparent strata. And indeed this is not an absurd statement and incapable of conveying information, as Mr. Ericsson seems to suppose.

First of all we must admit that a gas exposed to a radiating source does not always attain the same temperature as a solid body. It is obvious, for instance, that the temperature of the free air at the top of a mountain is a great deal lower than the temperature of the thermometer exposed to the sun. This is due to the small absorptive power of the gas. Therefore, at the boundary of the solar atmosphere the temperature of the transparent gas may be a good deal lower than that of a solid thermometer (if by hypothesis it could preserve there its solidity). It is besides not incorrect to say that the different successive strata may add their own radiation, so that by two, three, or more radiating strata we could obtain a higher temperature than by a single one. At least this has been understood, even by M. Respighi, who, however, is of the same opinion as Mr. Ericsson about the exaggeration of my result. The integral effect of all the strata that contribute to this elevation would be the indication of the thermometer, which may be higher than the temperature of a single outside stratum subjected to external radiation.

Mr. Ericsson says that it is of no consequence whether the sun's photosphere belongs to the class of active or sluggish incandescent radiators. I think, however, this point to be very important. Since we cannot experimentally determine the temperature of the sun except by using its radiating power, it is very interesting to take into account this element as very substantial. Very few, indeed, will allow that which Mr. Ericsson takes for granted, that

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